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Interactive Comment

Interactive comment on "An assessment of climate state reconstructions obtained using particle filtering methods" *by* S. Dubinkina and H. Goosse

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We would like to thank the Anonymous Referee 3 for his helpful remarks and suggestions, which were taken into account in the revised version.

I have one more substantial suggestion, which is that the performance should be evaluated against a no-assimilation solution. The reason for this is that the high correlations found between estimate and truth are (as the authors themselves note) largely due to the strong externally-forced trend, which the model follows independently of the assimilation. One possible approach might be to calculate the correlation of residuals from the mean no-assim result. Alternatively, a conventional calculation of skill relative to

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the no-assim baseline.

Response: Comparison with simulations without data assimilation is added to the revised version.

Title: You are really assessing methods, not reconstructions. "An assessment of particle filtering methods for climate state reconstructions"?

Response: In the revised version it is changed to “An assessment of particle filtering methods and nudging for climate state reconstructions”.

p44 l15 "*such* as atmospheric..."?

Response: Done.

l15-18 seems internally inconsistent - it is hard to see that opposite pattern (I presume this means negative correlation and skill) can be satisfactory for any application.

Response: This is taken into account in the revised version: “When reconstructing variables that are not directly linked to the pseudo-observations such as atmospheric circulation and sea surface salinity, the particle filters have equivalent performance and their correlations are smaller than for surface air temperature reconstructions but still satisfactory for many applications. The nudging, on the contrary, obtains sea surface salinity patterns that are opposite to the pseudo-observations, which is due to a spurious impact of the nudging on vertical exchanges in the ocean.”

p45 l8 "biased" is a poor choice of word. "limited"? It may be worth mentioning the assumption of gaussian likelihood, which although you also adopt in this paper, is not necessary for particle filtering.

Response: This is taken into account in the revised version: “These methods, however successful, are limited in the sense that the analysis is linearized and thus the methods assume Gaussian distributions. There exists an ensemble-based data-assimilation method that does not make such an assumption. It is particle filtering.”

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l24 This wording is imprecise - the variance of weights increases over time, irrespective of ensemble size. Also, so long as the degeneracy only takes place over several assimilation cycles, resampling is a reasonable solution. The approaches of van Leeuwen and others increase the efficiency.

Response: This is taken into account in the revised version: “Particle filtering has no assumption of gaussianity, uses a full nonlinear model to propagate the particles, but unfortunately, suffers from the "curse of dimensionality" [Snyder et al .2008], meaning that for a high-dimensional system particles (ensemble members) tend to drift apart during their forward evolution leading, consequently, to large variance in the corresponding importance weights. If the ensemble size is small, after a few data-assimilation cycles all but one of the particles have importance weights close to zero, and an ensemble that has collapsed to a single particle can no longer approximate the probability distribution function of the state.”

p48 l23 "no information". But you generated the ensemble somehow. Probably you intend to sample randomly from the attractor (requiring sufficient spin up from perturbed initial conditions), which could be clearly stated here.

Response: This sentence is removed to avoid confusion.

p49 Perhaps a couple of sentences to explain where the likelihood comes from, ie the probability of a particular set of observations, conditional on the underlying state. I think it’s worth clarifying that the likelihood is not really an assumption of the algorithm or often even the data assimilation researcher, instead being determined by the observation uncertainties.

Response: This is taken into account in the revised version: “$p(d^n|\psi^n)$ is the likelihood of the observations given the model state and it is related to the observation uncertainties.”

p50 l12 I think nudging is common enough in atmospheric modelling, especially these

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days when using regional models for dynamical downscaling. But not, perhaps, for initialising predictions (which you mentioned previously). Suggest rewording here.

Response: Nudging is used in coupled models quite widely, and the component over which nudging is performed is usually the ocean. Therefore, we modified the text in the revised version: "In our experiments, we nudge sea surface temperature, since in general circulation models nudging is usually performed over the ocean, e.g. [Swingedouw et al. 2012]."

p50 l13 I like the terminology "implicit particle filter" for the generic approach (several papers use this phrase), though in the particular case, "nudging particle filter" might be more precise.

Response: It is changed to the nudging proposal particle filter in the revised version.

p51 l12,14 Notation seems unclear to me. Haven't you already defined the transition densities as having non-zero mean?

Response: There is no contradiction. The transition densities have also non-zero means on p51 l12,14 of the discussion paper. To be more precise, the transition density was defined as the density of $\hat{\xi}^n$ with mean $f(\psi^{n-1})$. Mean of the transition density on "p51 l12" of the discussion paper is $f(\psi^{n-1})$. So it is the same mean.

The proposal transition density was defined as the density of ξ^n with mean $f(\psi^{n-1}) + \alpha H^T(d^n - H(\psi^{n-1}))$. If we rewrite the proposal transition density on "p51 l14" of the discussion paper in terms of ψ^n, we get the same mean, $f(\psi^{n-1})+\alpha H^T(d^n - H(\psi^{n-1}))$.

p53 l13 $(0.5C)^2$ would be less ambiguous.

Response: Done.

p55 l20 As mentioned earlier, I strongly recommend that all calculations are performed relative to the no-assim run (i.e. mean of no-assim ensemble). This would enable a more direct assessment of the performance of the methods without conflating this with the forced response.

Response: Done.

p57 The description here seems a bit garbled. You ignore data north of 60S in order that this does not introduce too many degrees of freedom and lead to degeneracy, right?

Response: This is taken into account in the revised version: “We assimilate the sparse pseudo-observations over the area southward of $60\mathrm{S}$ in order to decrease the number of degrees of freedom and avoid degeneracy”

Interactive comment on Clim. Past Discuss., 9, 43, 2013.